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Bloomfield thinks that the Vedic period really is a Brahmanic age, and is "saturated with Brahmanic ideas and Buddhistic formalism." On the other hand, the contention of Brunnhofer that the Rig Veda was a product of Aryan thought on the shores of the Caspian before the descent into India, is rightly pronounced 'extravagant' by Hopkins. Both Dr. Menzies and Professor Hopkins leave the battle to be fought out by others, and the question whether there was a pre-Vedic religion in which the Manes were worshipped is still not settled, and perhaps will never be. Professor Hopkins concerns himself only with the history of Hindu religions beginning with the Rig Veda; and in the Vedic age the worship of Manes and that of natural phenomena were certainly distinct. And he finds no evidence in the Rig Veda that nature worship was developed out of ancestor worship.

The discussion will probably continue concerning the primitive or secondary character of the religion of the Rig Veda. When, however, we consider the vast antiquity of man lying behind the Aryan peoples, even before their descent into the Punjab, it would seem that the Vedic religion was an advance upon primitive conceptions, although for purposes of their own the intelligent poets of the Vedic age set forth a simple nature worship. It seems difficult to abide by the view of Müller, Muir, and others — eminent scholars as they are — that the poets of the Rig Veda represent a childlike and primitive age, and that the hymns, although expressing the worship of natural phenomena, were not written by an advanced school of poets. It seems an impropriety to press the hymns of the Vedic age into service to prove that nature worship was the root of religion. Without doubt it was the first form of primitive worship, but the evidence for it should be sought elsewhere than in the Rig Veda.

Professor Hopkins' book, the first of a series of manuals upon the history of Religion, will be consulted by students with profit. The bibliography appended is as copious as the index is meagre.

CHARLES MELLEN TYLER.

Biological Lectures, delivered at the Marine Biological Laboratory of Wood's Holl in the Summer Session of 1894. Boston, Ginn & Co., 1895. — pp. vii, 287.

This handsome volume consists of twelve lectures on the burning questions of current biological science by distinguished American biologists, together with a translation of the introductory article (by Professor Roux) of a new German biological "Archiv."

The central problem which nearly all the essays attack is the mystery of the propagation of life, itself regarded as the avenue leading to the understanding of the nature of life generally, and the perennial interest which that problem possesses for every thinking mind must excuse a notice of such highly technical work in the technical journal of another science. A philosopher will not, of course, presume to criticize the work of the experts who instruct him, but he may profitably mark their methods, estimate the purport of their results and enrich his own conception of the world by the material which their industry has brought to light. And from the present volume the philosopher may learn much.

Not that all the essays in it lend themselves equally to philosophic reflection, but that they are all fascinating as illustrations of the working methods of a science. For the reader is, as it were, taken away from the polished periods of the popular lecture, and led into the laboratory where persistent ingenuity is grappling with the puzzling profusion of Nature's material, and made to realize how difficult and how great is the work of extorting an answer from Proteus. And so we are spared all the shallow pretensions to infallibility which are so apt to disfigure the statements of the mere popularizer. The authors are not afraid to exhibit the 'ragged edge' of their science, to point out the difficulties and uncertainty of theories and the inadequacy of the recorded facts, and by taking their reader into their confidence, they have adopted the surest means of exciting his sympathy.

That is one impression which, I believe, every unbiassed reader of this volume will carry away. Another is that American biologists are fully aware that in science the chief value of a theory lies in its verification, and that the true scientist is not merely tireless in seeking out the facts that test his theories, but always ready to consider what theory best accords with the facts.

And it is especially refreshing to record Professor Osborn's frank protest (p. 79 f.) against the vast amount of abstract reasoning from assumed data, paraded on both sides of the current controversy about the inheritance of acquired characteristics. This practice has recently gone so far that one view has actually been pressed upon us as the only thinkable view, and that we have been bidden to infer that Natural Selection is the *only* cause of Evolution *because* it has been admitted to be a *vera causa*. Professor Osborn's lucid article on *The Hereditary Mechanism and the Search for the Unknown Factors of Evolution* is admirably calculated to clear up such confusions of thought. This and the fact that its subject has the greatest general interest mark it out for special notice.

Professor Osborn considers that the destructive criticism and reductions *ad absurdum* which have abounded in the Spencer-Weismann controversy would open "a retrograde chapter in the history of science," if they should lead to the acceptance of laws resting so largely on negative reasoning. Hence the most important outcome of the discussion has been the negative conviction that neither the Lamarckian nor the Weismannian theory has been established by direct evidence, and that such evidence is greatly needed. Consequently there is arising among biologists a feeling that they are still on the threshold of their problem and a readiness to consider all relevant working theories. These Professor Osborn classifies as (1) Darwin's Survival of the Fittest, which alone may be regarded as "absolutely demonstrated as a real factor, without committing ourselves as to the origin of fitness"; (2) Buffon's factor of the direct influence of the environment; (3) Lamarck's of the active and adaptive response of the organism to the environment; (4) St. Hilaire's evolution *per saltum;* (5) Nägeli's assumption of a definite and continuous direction of the course of Evolution. In the observed results all these factors may coöperate, and the question is how to analyze those results and to determine the part possibly played by each. With Bateson, Professor Osborn thinks that a study of the facts of Variation is most needed.

But the facts must be analyzed before they are studied. Some variations may be *ontogenic*, due to circumstances in the life of the individual, others may be *phylogenic*, and affect the life of the race. Again, they may be, with respect to the time-series, retrogressive, progressive, or neutral (*i.e.*, mere individual anomalies). Each of the former classes must be subdivided according as they exhibit (*a*) repetition of parental type, reversion from parental to present race type, or reversion to present race type, and (*b*) individual variation from parental type, from race type, or racial variation from race type. The neglect of such distinctions has led to inclusion in the evidence of many merely individual 'sports,' which have no real bearing on the origin of species, and have unduly supported Darwin's assumption of merely Fortuitous Variation, and Bateson's of Discontinuous Variation, while swamping the evidence for Nägeli's factor. Similarly the evidence for Buffon's factor has been overvalued: it has so far been shown capable of producing only reversion to ancient types, or (under novel conditions) individual variations. But such novel conditions acting in the early history of the individual may very likely produce evolution *per saltum*.

On the other hand the evidence for *gradual phylogenic* variation is very strong in palaeontology. "The palaeontological series exhibit no evidences of fortuity in the main line of evolution. New structures arise by infinitesimal beginnings at definite points. In their first stages they have no 'utilitarian' or 'survival' value. . . ." "The main trend of evolution is direct and definite throughout, according to certain unknown laws and not according to fortuity" (p. 96). This "does not positively demonstrate Lamarck's factor, because it leaves open the possible working of some factor at present unknown," but it does set aside the "all-sufficiency of Natural Selection." Thus while Buffon's and Lamarck's factors yield no theory of Heredity, Neo-Darwinism offers an inadequate explanation of Evolution. "If acquired variations are transmitted, there must be some unknown principle in Heredity; if they are not transmitted, there must be some unknown factor in Evolution" (pp. 98, 99).

And so Professor Osborn concludes a very suggestive paper with the "last word" that "we are entering the threshold of the Evolution-problem instead of standing within the portals," and this remark the present reviewer cannot but cordially assent to and welcome. For it shows that scientists are at length awakening to a fact which he has for some time urged [*cf.*, *e.g.*, PHILOSOPHICAL REVIEW, II, 587, 588; IV, 203], *viz.*, that the current scientific theories of Evolution do *not* account for the fact of Evolution. That is, they contain no reason why a world which exhibits the tendencies they recognize should be progressively evolving or changing as a whole. Darwinism regards Natural Selection as the cause of Evolution; but Natural Selection is just as active where there is no change in species proceeding. Lamarckism supposes that the organism adapts itself to its environment; but why should these efforts result in a progressively graded series of beings? Mr. Spencer enumerates a variety of factors tending to greater differentiation: but he omits to state why they should prevail over the contrary tendencies. And so with the rest: they all take the *empirical progressiveness* of Evolution for granted as a fact which the theory need not explain.

The only theory that could, to any extent, explain the fact would seem to be Nägeli's, and it is a hopeful sign to find Professor Osborn regarding it with so much favor. There is, of course, still a considerable gap between the recognition of a definite "line" of Evolution and its interpretation as due to intelligent direction, but scientific thought is evidently travelling upon lines which may eventually surprise those who hastily assumed that teleological principles were disposed of forever. And so far from its being unthinkable and unknowable, as Weismann will have it, it seems possible that by the time Science really knows the facts of Variation and Development it may be able to give a fairly complete account of the character and limits of the teleological factor in Evolution. Even hitherto, teleology seems to have been ruled out chiefly because it was supposed to involve an appeal to an infinite and inscrutable power. But Kant's proof of the impossibility of a teleological argument conducting to an infinite, ought to have liberated the scientist from this fear; and so it is conceivable that he may after all come to recognize a long-lost brother in the philosopher who, taking his stand on the *ἄνθρωπος μέτρον*, all along maintained that a world which was commensurable with the human reason at all could not but exhibit the inmost characteristic of that reason, and prove accessible to the conception of a purposive intelligence.

F. C. S. SCHILLER.